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LORD LINDSAY, M.P., F.R.S., President, in the Chair.

William Charles Armstrong, Esq., Wanderers' Club, Pall Mall, S.W.;

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Thomas W. Bithrey, Esq., 45 Stepney Green, E.;

The Rev. James Law Challis, M.A., Vicarage, Stone, near Aylesbury;

George Howard Darwin, Esq., M.A., Trinity College, Cambridge;

Henry T. Vivian, Esq., 34 Wellington Road, Camberwell, S.E.; and

Robert Rumsey Webb, Esq., M.A., St. John's College, Cambridge;

were balloted for and duly elected Fellows of the Society.

On the Adjustment to Position of an Equatoreal Telescope. By the Rev. James Pearson, M.A., F.R.A.S.

Having recently had occasion to set up for the first time an Equatoreal Telescope, I adopted a method which I have not seen described elsewhere, and which has proved very successful. It is independent alike of meridian observations and spirit-levels, and for distinction's sake I may term it the "method of co-ordinates." An upright pyramidal stone of great height compared with the circumference of its base having been firmly embedded in the ground, with its principal faces north and south

approximately, the mounting of the Equatoreal was firmly attached to it by means of bolts, screws, and clamps.

The mounting, which, as well as the telescope, was made by Cooke & Sons, of York, consists of a polar axis carrying a single hour-circle, and a cradle-piece so constructed that the telescope may be separately affixed or withdrawn at pleasure. The cradle-piece forms an appendage to the declination-axis, at right angles to the former, which carries the declination-circle.

The telescope, then, being placed on its mounting, both were approximately set in position, *i.e.* the polar axis in the direction of the pole and the axis of the telescope with its plane of motion coincident with the plane of the meridian. This being effected, the telescope was pointed to a star, and then the mounting was moved about on its bearings by means of foot-screws, and round a vertical screw, forming an axis, until the declination-circle indicated the true North Polar Distance of the star when in the centre of the field of view. A marine chronometer, tested to Greenwich Mean Time by means of a Dipleidoscope, was employed to determine the Right Ascension of the meridian of the place at a given instant, and thence, by subtraction of the star's Right Ascension, its Hour-angle was found for the same instant. For convenience, the instant selected was the commencement of some hour on a common watch, its error being compared with the standard chronometer, and thus the Hour-angle of the star at any following instant was obtained at a glance by adding the minutes and seconds elapsed on the watch since the commencement of the hour nearest to it.

This done, two coordinates were obtainable for the same instant, viz. the true North Polar Distance of the star, and its true Hour-angle; and thus, by moving once more the mounting on its bearings, the hour-circle was made to indicate the true Hour-angle, and then the position was made permanent by the screws.

The adjustment was found so far satisfactory that planets and stars could be made visible in the day time by its means, which is a competent test of its accuracy.

Note on the Difference of Variation of Gravity at Revel and St. Petersburg; and on Grischow's Pendulum Observations at other Stations. By Major J. Herschel, R.E., F.R.S.

The difference to which I wish to draw attention is that exhibited on comparing the observations at the two stations named, by Grischow* in 1757, and by Sawitsch in 1865.

As such a comparison will seem at first sight derogatory to the modern observation, I will give my reasons for thinking that the more ancient are not devoid of reliance.

* *St. Petersb. Acad. Sci. Novi Comm.* vii., pp. 447–451.

Grischow employed two pendulums, one of which had seen service with La Condamine (who had had it made) in Peru, and the other with La Caille at the Cape of Good Hope. The former was swung in connection with machinery which counted its oscillations; the other was swung free, and its rate was determined by coincidences with the pendulum of an astronomical clock, every twelve or thirteen minutes.

Grischow observed these two at five stations, and were it not for the unaccountable difference between his results and those of Sawitsch, I venture to say that they should enlist immediate confidence on the sole ground of their concurring testimony.

I do not propose to go into any details of reduction here; but in order to show the grounds of this confidence, I give the vibration-numbers, reduced to 13°·3 R. or 62° F., which is about the average temperature of the whole series. The observations at Arensburg extended over nearly a twelvemonth, and supply good temperature factors, viz. 1·1 and 0·7 respectively for 1° R. For the purpose of comparison it is necessary to reduce the one to the unit of the other, or both to a common unit. I prefer the latter method, and adopt the Equatoreal Seconds Pendulum. By means which it is not necessary to describe, I estimate that the one (A) would make 98758·0, and the other (B) 86350·0 vibrations, in air, at 13°·3 R., at the Equator, or near it; and these are the numbers I use to reduce to the terms of an Equatoreal Seconds Pendulum vibrating 86400.

The following short table exhibits the data* and comparison. (I have added La Caille's observations at Paris as quoted by Grischow,* but reduced to 13°·3 R.):—

	A	B	A	B	A−B
St. Petersburg	98950·1	86519·9	86568·0	86570·0	−2·0
Revel	941·8	510·2	560·8	560·3	+0·5
Arensburg	937·4	504·9	556·9	555·0	+1·9
Pernau	940·6	509·4	559·7	559·5	+0·4
Dorpat	939·6	510·6	558·9	560·7	−1·8
Paris	900·5	453·1	524·7	503·2	...

There is no selection of observations here. The figures represent the whole of Grischow's published observations; and I think that on the face of it they are entitled to confidence. I exhibit them solely to justify the inquiry which I shall now proceed to make being listened to. I base nothing upon them as yet.

Returning now to the *un*reduced observations at St. Petersburg and Revel, these are:

At St. Pet.	By A 98947·1	At 16·0 R.	By B 86516·9	At 17·6 R.
„ Revel	„ 941·8	„ 13·3 „	„ 510·4	„ 13 „
Diff.	+5·3	+2·7	+6·5	+4·6

* See also *St. Petersb. Acad. Sci. Nova Acta*, vii., p. 215 *et seq.*

or with the temperature factors 1·1 and 0·7 respectively.

St. Pet.—Revel +8·3 and +9·7

which, when reduced to seconds, become

+7·3 and +9·7

agreeing of course with the former reduction.

Now Sawitsch's observations, as last published (see *Monthly Notices* for March 1879)—observations which I am unwilling even to seem to question—give the following:

Length of seconds pendulum at St. Petersburg	441·0254
„ „ at Revel	441·0125

This difference corresponds to a change of Vibration-number of an Equatoreal Seconds Pendulum

$$= +\tfrac{1}{2}\frac{\cdot 0129}{441\cdot 0}.86570 = +1\cdot 3 \text{ sec.}$$

I am quite unable to discover any reasonable explanation of the discrepancy. Naturally Sawitsch's has much the higher probability; and I have therefore examined the older observations carefully. I argued thus: Grischow's results are with entirely different pendulums, it is true; but do they depend on the same clock rates? Yes. Then the clock rate must be in error. In effect, I at once found an error of 10 sec. (see *St. Petersb. Acad. Sci. Novi Comm.*, vii., p. 451, l. 5) in the mean daily rate of the clock. But this proved to be only a misprint: the more correct rate had been used. Nor am I able to believe readily in the clock rate being wrong. The swings of A extend continuously over many days at both stations, the clock is rated by transits daily of the Sun and stars, and an undetected error of 6 or 8 seconds a day is scarcely conceivable.

I submit the difficulty therefore for the scrutiny of others, with the hope that some solution will be found.

A like difficulty attends—but in a less degree—the results at Dorpat. To show this I will suppose that my table above is accepted, subject to a possible constant but unknown correction to each of the second pair of columns, to allow for the provisional character of the adopted equatoreal numbers. I now show the same figures at four stations, of which the Local-numbers—by which term (with capitals) I denote the Vibration-numbers which we otherwise know, with more or less certainty, would be shown by an Equatoreal Seconds Pendulum—are annexed for comparison.

		A	B	Mean.	Loc.'No.
1757	St. Pet. (Grischow)	86568·0	86570·0	86569·0	86569·5
1757	Revel „	60·9	60·3	60·6	68·2
1758	Dorpat „	58·9	60·7	59·8	64·0
1756	Paris (La Caille)	24·7	...	24·7	24·2

To guard against any suspicion that these figures—the very remarkable agreement of which with each other in certain respects, and equally remarkable disagreement in others, is patent—are the result of discriminative selection (a kind of survival of the fittest, in fact), I beg to say that they are the direct consequence of adopting the Equatoreal-numbers 98758·0 and 86350·0. Each pendulum *had* an equatoreal vibration number which was not known. We have a right to assume any number for any pendulum, in any given condition, which will best satisfy the observations. I propose these, and show how well they fit the observations. That is evidence in their favour. If they are right they are a key to help to discriminate and to indicate facts. Thus it appears, beyond any doubt, that the condition of B, when in Grischow's hands, was not the same as when in La Caille's; and the Vibration-number is accordingly dropped, as not belonging to the differential series.

I have gone further in this examination than I intended, in the hope of finding other reasons, in the discordance *inter se* of the results, for their non-appearance in any collection of pendulum observations. That the Revel and Dorpat results are now apparently irreconcilable with the more recent results of 108 years later is true; but was this foreseen? And is there even now the *smallest* ground for doubting the Arensburg value?

Calcutta, May 24, 1879.

Note on the Semi-diameter of the Moon.

By E. Neison, Esq.

The results obtained by Professor Pritchard from the measurement of the photographs of the Moon taken at Oxford, and given in the *Monthly Notices* for June, p. 447, are of considerable interest. Unfortunately, as Professor Pritchard gives no details of the method he employed, beyond stating that it is very similar to that employed by Wichmann (*Ast. Nach.*, 1847), it is impossible to say how far he has succeeded in eliminating the systematic errors which are incidental to this work. Professor Pritchard makes the semi-diameter of the Moon equal to

$$15\ 34'175.$$

From a careful discussion of nearly eleven hundred observations made with apertures of different sizes, at the Observatories of Greenwich, Oxford (Radcliffe), and Washington, I deduced the value

$$15\ 33'37 + 4''10 \left(1 + 0'70 \times \text{aperture in inches}\right).$$

For the eight-inch Transit-Circle of the Greenwich Observatory this corresponds to a semi-diameter of

$$15' \; 33''\cdot99,$$

the result from 350 measures being

$$15' \; 33''\cdot97.$$

For the five-inch Transit-Circle of the Radcliffe Observatory the formula gives a semi-diameter of

$$15' \; 34''\cdot28,$$

whilst the mean of over 200 measures, between 1862 and 1877, gives

$$15' \; 34''\cdot35.$$

It may be added that this empirical formula agrees closely with the theoretical formula, founded on the assumption that the differences in semi-diameter obtained from instruments of different aperture vary as the diffraction disks and the amount of light.

Applying this formula to the semi-diameter of the Moon in a 13-inch Reflector, the result is obtained of

$$15' \; 33''\cdot78$$

for the value of the semi-diameter. Adding the photographic irradiation, which my experiments indicate to be about $+''0\cdot30$, the resulting photographic semi-diameter should be

$$15' \; 34''\cdot08.$$

This agrees very closely with the result obtained by Professor Pritchard.

How far this coincidence is fortuitous cannot be said; for until we know the method by which these measures have been reduced, it is impossible to say how far the above result indicates the true photographic semi-diameter, or how far it may be vitiated by the effect of the inequalities on the limb. In the method employed by Wichmann the effect of these inequalities is not eliminated; and if Professor Pritchard has employed the same method they will not have been eliminated from the above result.

Professor Pritchard compares the remarkably accordant results obtained by himself with the results obtained by Wichmann, apparently with the view of showing that the results obtained by measuring photographs will be far superior to the results of measurements with the Heliometer. The comparison, however, fails completely. Professor Pritchard's column III. gives the difference between his own separate and mean values, whereas his

column IV. gives the result of comparing the separate Heliometer determination with the very faulty semi-diameter (Burckhardt's) employed in the *Berliner Jahrbuch* for 1844–1845. To render the comparison fair, Professor Pritchard should have compared his separate results with the semi-diameter from Burckhardt's Tables. He would then have found his separate results as utterly discordant as those of the Heliometer appear to be.

As Professor Adams has pointed out, Burckhardt's tabular semi-diameter is vitiated by a large error, an error which may amount to some $2''$, whilst his mean semi-diameter is more than $1''$ too small. Correcting Burckhardt's semi-diameter for the effect of these errors, all the large discordances in column IV. disappear, leaving the separate results of the Heliometer sensibly as accordant as those from the photographs.

Professor Pritchard remarks that the mean semi-diameter (Hansen's) employed in the *Nautical Almanac* appears to be

$$15'\ 34''{\cdot}10.$$

I do not know how Professor Pritchard has arrived at this conclusion, but it seems quite erroneous. In the *Nautical Almanac* Hansen's tabular semi-diameter is employed unaltered. Hansen clearly states, in the introduction to his Tables, that he adopts $57'\ 0''$ for the mean value of the Moon's horizontal equatoreal parallax, and therefore, as Table XXII. shows, the mean value of the tabular semi-diameter is

$$15'\ 33''{\cdot}47,$$

no less than

$$0''{\cdot}63$$

smaller than supposed by Professor Pritchard. Instead, therefore, of the photographic semi-diameter differing from the *Nautical Almanac* semi-diameter by the very small amount of

$$+0''{\cdot}075,$$

it differs by the rather considerable amount of

$$+0''{\cdot}705,$$

or nearly ten times as much.

As Hansen's Tables are not very intelligible on a mere cursory inspection, it is important that this correction should be made, to prevent erroneous ideas as to the real value of Hansen's semi-diameter.

Comparison of Hansen's Coefficients of the Moon's Latitude with those of Plana and others. By R. Wilding, Esq.

The values of the coefficients of the terms in the expressions for the coordinates of the Moon having now been found by a number of independent calculators, I venture to suggest that, for the practical purpose of forming lunar tables, perhaps the best values to be adopted would be those which are found to agree most nearly, *especially* if obtained by methods entirely different in principle. When the results, under such circumstances, *very* nearly agree, it seems highly probable that they are correct except as regards variations which might be accounted for by the fact that different elements of the orbit are assumed in their calculation. In cases where the coefficients are given *literally* the latter discrepancy may be made to disappear.

A comparison is made by Hansen, in his "*Darlegung &c.*," between his coefficients of the Moon's longitude and those found by Plana and Damoiseau; but I am not aware that any similar one is made of his latitude with that of other writers, and I have therefore endeavoured to do so in the present paper.

For this purpose, Hansen's expression for the latitude has to be reduced to a series of terms having for arguments angles increasing *as* the time. His mode of deducing the latitude is peculiar. First, a kind of *mean sine* of the latitude is taken equal to (sine *constant* inclination) × (sine of *true* distance of Moon from *true* ascending node), and this is afterwards corrected by the addition of a number of small equations, the result being the *true* sine of the latitude. From this the latitude itself is then obtained.

From the above explanation it will be seen that the reduction of the whole to terms having arguments varying *as* the time is a matter of some complexity, and one in which there is great liability to error; so I must request that *too much* reliance should *not* be placed upon my figures, although I cannot help saying that they *appear* very correct, and have been honestly worked out.

The very remarkable agreement with those of Delaunay which they present induces me to give his coefficients also for comparison. It is true that both Hansen and he used the same elements (Airy's); but the processes employed were so vastly different that the close accordance of the results seems to indicate that they are both extremely correct.

Both Clausen (*Astron. Nach.*, No. 406) and Pontécoulant (*Théorie Analytique*) have compared Burckhardt's coefficients with those of Damoiseau and others; but, as Burckhardt's coefficients of *precisely* the same arguments differ very considerably as given by these two authorities, it is clear that one or both reductions are erroneous. Pontécoulant's reduction of Burckhardt's latitude seems certainly wrong, and has the effect of making it appear that Burckhardt's tables of latitude are much worse than they really are.

The arguments are those of Delaunay.

D = ☽ − ☉
F = ☽ − ☊
l = ☽'s mean anomaly
l' = ☉'s „ „
} All mean angles.

Argument.	Delaunay.	Hansen.	
	"	"	
F	18461·3	18461·6	
$F+l$	+ 1010·2	+ 1010·3	
$F-l$	− 999·8	− 999·9	
$2D-F$	+ 623·6	+ 623·6	
$2D+F-l$	+ 199·3	+ 199·6	
$2D-F-l$	+ 166·4	+ 166·6	
$2D+F$	+ 117·2	+ 118·4	Dam. 117″·3; Plana 117″·6; Pont. 117″·5.
$F+2l$	+ 62·0	+ 62·0	
$2D-F+l$	+ 33·4	+ 33·0	
$F-2l$	− 32·3	− 31·8	
$2D-F-l'$	+ 29·7	+ 29·7	
$2D-F-2l$	+ 15·3	+ 15·9	
$2D+F+l$	+ 15·1	+ 15·1	
$2D-F+l'$	− 12·2	− 12·2	
$2D+F-l-l'$	+ 9·0	+ 8·9	
$2D-F-l-l'$	+ 7·5	+ 9·3	Dam. 7″·4; Plana 8″·2; Pont. 8″·3.
$2D+F-l'$	+ 8·0	+ 8·0	
$F+l-l'$	+ 4·6	+ 6·8	Dam. 6″·8; Plana 6″·4; Pont. 6″·3. (I may have taken out Delaunay's coeff. wrongly.)
$4D-F-l$	+ 6·5	+ 7·7	Dam. 6″·3; Plana 6″·6; Pont. 6″·6.
$F-l+l'$	− 5·4	− 5·6	
$F+l'$	− 6·6	− 6·5	
$F+D$	− 5·3	− 5·5	
$3F$	− 6·3	− 6·3	
$F-D$	+ 4·8	+ 4·8	
$F+l+l'$	− 5·3	− 5·3	
$F-l-l'$	+ 5·0	+ 5·1	
$F+3l$	+ 4·0	+ 4·0	
$4D-F$	+ 3·7	+ 3·6	
$4D+F-l$	+ 2·8	+ 3·0	
$3F-l$	− 2·8	− 2·8	
$F-l'$	+ 4·9	+ 4·9	
$3F-2D$	− 2·2	− 3·4	Dam. −2″·3; Plana −2″·4; Pont. −2″·3.
$4D+F-2l$	+ 2·3	+ 2·4	

Argument.	Delaunay.	Hansen.	
	"	'	
$2D-F-3l$	+ 1·5	+ 2·1	
$2D-F+2l$	+ 2·1	+ 2·2	
$2D+F-2l$	− 1·7	− 1·4	
$2D-F+l-l'$	+ 1·8	+ 0·3	Dam. 1″·6; Plana 1″·6; Pont. 1″·6.
$2D+F+2l$	+ 1·5	+ 1·5	
$2D+F+l'$	− 1·2	− 1·2	
$2D+F-l+l'$	− 1·8	− 1·6	
$4D+F$	+ 1·1	+ 1·2	
$2D+F+l-l'$	+ 1·1	+ 1·1	
$F-D+l$	+ 0·4	+ 0·5	
$2D-F-2l'$	+ 1·1	+ 1·1	
$3F+l$	− 1·0	− 1·0	
$2D-F+l+l'$	− 0·8	− 0·8	
$F+2l-l'$	+ 0·7	+ 0·8	
$F-3l$	− 1·6	− 1·6	
$2D-F-l+l'$	− 1·2	− 0·9	Dam. −0″·8; Plana −1″·9 Pont. −2″·0.
$D+F+l$	− 0·6	− 0·7	

A close agreement is found to exist between the remaining small coefficients which are omitted. The coefficient of the term having for argument, Moon's true longitude, is also omitted, as I do not find that Delaunay has given his value of it.

Note on the Ellipticity of Mars, and its effect on the Motion of the Satellites. By Professor J. C. Adams.

One of the results of Professor Asaph Hall's able discussion of his observations of the satellites of *Mars* is to show that the orbits of both the satellites are at present inclined at small angles to the plane of the planet's equator. It becomes an interesting question to inquire whether this state of things is a permanent one. The plane of *Mars*' orbit is inclined to its equator at an angle of 27° or 28°. If then the planes of the orbits of the satellites retain constant inclinations to the orbit of the planet, as they would do if the Sun's disturbing force were the only force tending to alter those planes, their inclinations to the plane of *Mars*' equator, and still more their inclinations to each other, would in time become considerable.

In No. 2280 of the *Astronomische Nachrichten*, Mr. Marth has found the motions of the nodes of the orbits of the satellites on the orbit of the planet due to the Sun's action, and he con-

cludes that, if there is no force depending on the internal structure of *Mars* which counteracts or greatly modifies the Sun's action, the nodes of the orbits will be in opposition to each other a thousand years hence, when the mutual inclination of the satellites' orbits will amount to about 49°.

In this case the near approach to coincidence between the planet's equator and the planes of the orbits of the satellites, which is observed to exist at the present time, would be merely fortuitous; but this appears *à priori* to be very improbable.

It is well known that, if there were no external disturbing force, the ellipticity of a planet would cause the nodes of a satellite's orbit to retrograde on the plane of the planet's equator, while the orbit would preserve a constant inclination to that plane. Laplace has shown that, when both the action of the Sun and the ellipticity of the planet are taken into account, the orbit of the satellite will move so as to preserve a nearly constant inclination to a fixed plane passing through the intersection of the planet's equator with the plane of the planet's orbit, and lying between those planes, and that the nodes of the satellite's orbit will have a nearly uniform retrograde motion on the fixed plane. The angles which this fixed plane makes with the planes of the planet's equator and its orbit respectively will depend on the ratio between the rates of the above-mentioned retrogradations of the nodes produced by the Sun's action and by the ellipticity of the planet. If the latter of these causes would produce a much slower motion of the nodes than the former, as in the case of our Moon, the fixed plane will nearly coincide with the planet's orbit; but if, as in the case of the inner satellites of *Jupiter*, the ellipticity of the planet would produce a much more rapid motion of the nodes than the Sun's action, then the fixed plane will nearly coincide with the planet's equator.

The ratio of the motion of a satellite's node to that of the satellite itself, when the Sun's action is the disturbing force, varies, *ceteris paribus*, as the square of the satellite's periodic time, that is as the cube of its mean distance from the planet. On the other hand, the ratio of the same two motions, when the ellipticity of the planet is the disturbing cause, varies inversely as the square of the mean distance. Hence, for different satellites of the same planet, the motion of the nodes caused by the ellipticity will bear to the motion caused by the Sun's action the ratio of the inverse fifth powers of the mean distances.

Now, the distance of the inner satellite of *Mars* from the planet's centre is only about $2\frac{3}{4}$ radii of the planet, a greater comparative proximity than is known to exist elsewhere in the Solar System, and the distance of the outer satellite from the same centre is only about 7 radii of the planet, while the periodic times of both are very small compared with the periodic time of *Mars*. Hence the effect of a given small ellipticity of *Mars* on the motion of the nodes of the satellites will be greatly magnified.

It is true that the ellipticity of *Mars* is still unknown, and is probably too small to be ever directly measurable; but we are not without means of determining, within not very wide limits, its probable amount, and we shall presently see that, in all probability, in the case of both the satellites the motion of the nodes produced by the ellipticity greatly exceeds the motion caused by the Sun's action, so that the fixed planes for both satellites are only slightly inclined to the planet's equator.

From measures of the planet's diameter and of the greatest elongations of the satellites, combined with the known time of rotation of *Mars* and the periodic times of the satellites, it is found that the ratio of the centrifugal force to gravity at *Mars*' equator is about $\frac{1}{225}$. Hence it follows that if the planet were homogeneous its ellipticity would be about $\frac{1}{176}$. If, instead of the planet being homogeneous, its internal density varied according to the same law as that of the Earth, so that the ellipticity would bear the same ratio to the above-mentioned ratio of centrifugal force to gravity at the equator as in the case of the Earth, then the ellipticity would be about $\frac{1}{228}$. In all probability the actual ellipticity of *Mars* lies between these limits.

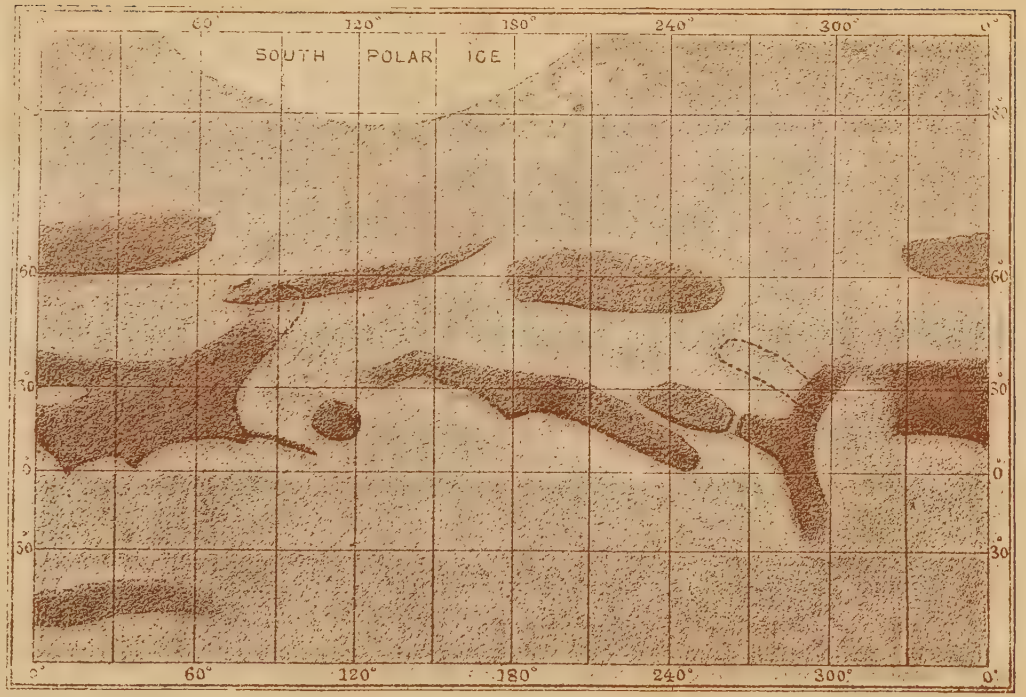
The following Table shows the annual motions of the nodes of the two satellites, caused by the Sun's action and by the planet's ellipticity respectively, for the above values of that ellipticity, and also for the ellipticity $\frac{1}{118}$, which has been deduced from Professor Kaiser's observations, although I have no doubt that this value is too great. The Table likewise contains the corresponding inclinations of the fixed planes, so often mentioned above, to the planet's equator.

Satellite I.			*Satellite II.*		
Annual motion of the node due to the Sun's action, 0°·06.			Annual motion of the node due to the Sun's action, 0°·24.		
Supposing ellipticity =			Supposing ellipticity =		
$\frac{1}{118}$	$\frac{1}{176}$	$\frac{1}{228}$	$\frac{1}{118}$	$\frac{1}{176}$	$\frac{1}{228}$
the annual motion of the node due to that ellipticity will be			the annual motion of the node due to that ellipticity will be		
°·333	°·182	°·113	°·134	°·73	°·45
Corresponding inclinations of fixed plane to planet's equator:			Corresponding inclinations of fixed plane to planet's equator:		
″	″	″	′	′	° ′
17	31	50	27	50	1 19

From this it may be inferred that the orbit of the 1st satellite preserves a constant inclination to a plane which is inclined less than 1′ to the plane of *Mars*' equator, and that the orbit of the 2nd satellite preserves a constant inclination to a plane which is inclined about 1° to the plane of the same equator.

The ellipticity will also cause rapid motions in the apses of

CHART OF MARS ON MERCATOR'S PROJECTION.



the orbits of the satellites, particularly in that of the first; and as this orbit appears from Professor Hall's determination to have a sensible eccentricity, it will be possible, by future observations, to determine the motion of the apse, and therefore the ellipticity of the planet. If further observations show that the orbits of the satellites are sensibly inclined to their fixed planes, the motion of their nodes will supply another means of determining the ellipticity of the planet.

On the Physical Configuration af Mars.

By Professor Wm. Harkness, U.S. Navy.

(*Communicated by authority of Rear-Admiral John Rodgers, U.S. Navy; Superintendent U.S. Naval Observatory.*)

I have constructed the accompanying map of *Mars* from eight sketches taken by myself between August 18 and October 18, 1877. More sketches would have been exceedingly desirable, but the unsteady state of the atmosphere prevailing during the opposition rendered it impossible to obtain them. The telescope employed was the 26-inch Equatoreal of this Observatory. Eye-pieces magnifying up to 400 diameters were tried each night, but it was usually found that a power of 175 gave the best result. This was due partly to the southern declination of the planet, and partly to the bad seeing before mentioned. After each sketch was finished, Professor Hall very kindly compared it with the planet, and in every case he agreed that the sketch showed all that was certainly visible upon the planet, and nothing more. As seen through the telescope, the colour of *Mars* was a golden yellow; except the polar spot, which was pure white, and the markings, which were a light indigo blue.

When laid down upon Mercator's projection the several sketches agreed well with each other, and resulted in the accompanying map; the latitudes and longitudes of which depend upon the Ephemeris given by Mr. Marth in the Royal Astronomical Society's *Monthly Notices* for April 1877, vol. xxxvii., pp. 301–304. The features indicated upon the map by dotted lines were seen only once, and appear inconsistent with other features which were seen oftener. The south polar spot was sensibly circular in form, and, according to my sketches, had a diameter of about $14\frac{1}{2}°$. Professor Hall determined the position of its centre to be latitude, 84°·8 south; longitude, 118° west.

Owing to the meagreness of the data upon which it is based, this map cannot be regarded as complete; but it is hoped that it may be greatly improved during the Opposition of next autumn. In its present state it somewhat resembles the map given by Dr. Kaiser in the third volume of the *Leiden Observations*.

U.S. Naval Observatory, Washington,
1879, *June* 17.

On a Photograph of the Solar Spectrum, showing Dark Lines of Oxygen. By Professor J. C. Draper.

(*Communicated by W. H. M. Christie, Esq.*)

I send by mail, with this letter, a photograph of a solar spectrum of the third order, in the vicinity of G. It is an original photograph of the diffraction grating spectrum itself. It has not been enlarged nor manipulated in any way whatever. It was taken on October 1, at the City College, and though the sky was not perfect, it is a good picture. If you think it good enough, I should be very much obliged if you would present it at a Meeting of the Royal Astronomical Society, for the inspection of members who take an interest in these matters.

The apparatus was essentially that described in the *American Journal of Science* for October 1868, page 256. It consisted of Heliostat with silvered glass mirrors, slit, silvered glass concave mirror, silvered glass Rutherfurd grating of over 17,000 lines to the inch, and a camera box. The light passed from the grating to the collodion plate, with nothing intervening except the glass bearing the scale, as described below. In the case of that part of the spectrum which does not bear the scale, nothing whatever except air intervened between the grating and the collodion on the plate.

The slit aperture with which the picture was taken was exceedingly fine, probably ($\frac{1}{3000}$) one three thousandth of an inch in width, and possibly less. Even with exposures of six and seven minutes, and a very sensitive collodion, the fineness of the slit has rendered it necessary to force the development of the image, hence come the minute spots on the film.

As the red of the second order overlaps this portion of the third order, one of your half-prisms, made for me in New York, was used for the separation of these orders in focussing. It was placed in the aperture of the focussing glass, and almost touched the clear glass plate of the camera, which occupied the position ordinarily held by ground glass.

The picture may be studied either as a positive or as a negative. In the first case, it is to be laid on a black background of velvet or cloth, and viewed by reflected light, when the dark lines represent the absorption lines and the white lines the bright or coloured spaces in the solar spectrum. In the second case it is to be held between the eye and the light of a white cloud (at night a light, with a ground-glass shade), and viewed by transmitted light. Thus examined with a suitable magnifying power, the clear spaces represent the dark lines of the spectrum, and the dark lines of photographic action, the bright parts of the spectrum. This must be borne in mind when making examinations in connection with the descriptions given hereafter.

The cross lines near the centre of the plate indicate the position of the line of no deviation for the apparatus described. The

capital letter G is written on the back of the plate bearing the collodion film. It is over the line G of the solar spectrum, and a little to the left of the line of no deviation. On the right of the line of no deviation, and beginning at the cross lines, five bands appear, the third and fourth containing what I believe to be dark lines of oxygen. The collodion film is protected by a second plate of glass, as the picture is not varnished. The two plates of glass are separated by a mat, made of blotting paper. The glass on which the collodion film rests, being ordinary photographic glass, is not suited for measurement of the spectrum lines by projection.

To examine the picture, hold it in such a position that the letter G reads properly. Then, using a moderate magnifying power ($2\frac{1}{2}$ to 3 inches focus), as before described, and viewing by transmitted light, a scale bearing divisions and figures will be seen above the line which runs the whole length of the photograph, and which divides it into two portions.

The manner in which this scale was introduced was as follows: On a slip of the flattest glass I could find, a scale of 200 half-millimetres was ruled. This was fitted in the plate-holder of the camera, in front of and close to the collodion film, with the ruled side of the plate looking toward the collodion. It was carefully adjusted to intercept one-half of the spectrum. Its shadow was thus cast with this half of the spectrum upon the collodion film, and so the scale was printed by the spectrum itself. The line which traverses the whole length of the spectrum is the edge of the glass upon which the scale is ruled. The scale I have introduced here merely as a means of indicating positions. Its real purpose is to verify readings of wave-lengths made by the projection process I have heretofore described.

The necessity of avoiding the intervention of anything between the grating and the collodion film is shown by the injury to definition and obscurity produced in the upper half of the spectrum by the glass on which the scale is ruled. In the case in question the obscurity is increased by vapours which arise from the collodion and condense on the glass scale.

To study the picture intelligently, take one of Dr. Rutherfurd's admirable photographs of this part of the spectrum; a series of five bands will be found on the less refrangible side of G. In the photograph I have used, their positions, as read by the scale attached, are as follows:—

1st	4312·60	to	4314·15	These readings may vary slightly in different photographs, owing to difference in stretching of the paper used for the photographs and the scale; but they serve to indicate the bands in question.
2nd	4314·60	„	4316·10	
3rd	4316·20	„	4317·95	
4th	4318·15	„	4320·00	
5th	4320·25	„	4322·15	

Take H. Draper's photograph, published in *Nature* for August 30, 1877, the five bands are easily identified, and the oxygen

lines in the spectrum below correspond to the third and fourth of these bands. In his article discussing the oxygen lines, he gives their wave-lengths as 4317 and 4319. In support of the opinion, that it is in this part of the spectrum that oxygen lines should be found if any exist, I refer to the diagram given in the *Observatory* for June 1879, page 47. The wave-length values of these oxygen lines, as given by the authorities there presented, are:—

Ångstrom.	J. C. Draper.	Plucker.	H. Draper.	Huggins.
4316·20 4316·95	4316·50	4317·	4317·	4318·
4318·85 4319·45	4319·75	4320·	4319·	

All agree in placing them within the limits of the third and fourth bands of Rutherfurd's photograph.

Take my photograph of the 3rd order, October 1, 1879. The five bands are easily identified, and present the following scale readings:—

1st	97·95	to	100·10
2nd	100·45	,,	102·35
3rd	102·75	,,	104·70
4th	105·40	,,	107·25
5th	107·70	,,	110·10

Remembering that the picture is now studied as a negative, and that the clear spaces are the dark lines of the solar spectrum, it will be seen that each of these five bands contains dark lines. In some instances also, and notably in the case of the line separating the 3rd and 4th bands, the separating line is double. An experience gained by the study of more than 200 photographs of this region leads me to think that many of the lines given in the following Table of the lines contained in these bands are really double:—

	Scale Number.		Scale Number.
Lines in 1st band	98·65 99·25 100·00	Separating lines	104·85 105·30
Separating line	100 30	Lines in 4th band	105·80 106·20 106·60 107·00
Lines in 2nd band	100 85 101·65	Separating line	107·50
Separating line	102·60	Lines in 5th band	108·10 108·60 109·70
Lines in 3rd band	103·20 103·55 104·20	Separating line	110·20

As regards the photographic intensity of these bands when compared with that of the lines between 70 and 80 of the scale, no one can have any doubt of its superiority in the latter case. To a careless examiner it may appear stronger in the first, but this is a deception caused by the great width of the bands.

In answer to objections that may be advanced against these lines in the 3rd and 4th bands as oxygen lines, on account of their lack of intensity, I submit the appearances offered by the picture. I would also suggest that, since both oxygen and nitrogen are really the only non-metallic gaseous elements that approach a permanently gaseous state, we should be prepared from this fact alone to expect them to produce only faint absorption lines in the solar spectrum.

It also seems to me that the picture really gives us the true spectrum of oxygen, for this region, under the conditions existing in the solar envelopes, viz. that it is a spectrum of six faint lines in two bundles, corresponding to those in the 3rd and 4th bands of the picture.

If we admit that the electric oxygen bands corresponding to this position are really composed of lines, and that the attenuated condition of gaseous oxygen in the Sun explains the faintness of oxygen absorption lines, we have no difficulty in accepting the opinion that oxygen is present in the solar envelopes, and that it acts there in the same manner as any other elementary body, and produces its proper dark absorption lines in the solar spectrum.

As regards the appearance of the picture from a photographic point of view, I would say that, if the slit is opened sufficiently, there is no difficulty in obtaining a more pleasing picture with a brief exposure. But, on opening the slit, purity of spectrum disappears, and as the slit is widened the lines fade away, until at last nothing remains but the five unlined bands, as is shown in H. Draper's photograph in *Nature* of August 30, 1877.

That there is nothing peculiar about this photograph of the 3rd order is shown by the fact, that in photographs of the 1st order the resolution of these five bands is foreshadowed, in those of the 2nd order it is partly accomplished, and in the 3rd order the dissection is completed, as you see.

New York, 1879, *Oct.* 9.

On the Working of the Speculum for Mr. Common's 37-inch Silver-on-glass Reflector. By Mr. G. Calver.

The Speculum recently finished for the 37-inch Reflector gave me an opportunity of coping with the difficulties to be encountered in making a large speculum of this kind. I have found some of these difficulties to be less than I anticipated; and I believe considerably larger instruments might be undertaken with a reasonable prospect of success.

There never was any doubt whether large glass specula could be as easily mounted as metal specula, for they have the great advantage of being lighter; but the chief question was whether they could be annealed so as to stand the usual treatment in working. If they would stand this, there is no fear of their durability when mounted.

To decide whether the speculum should be metal or glass was, to a certain extent, to venture a risky experiment. For my own part, I had reason to believe that large disks could be obtained properly annealed. Two firms had guaranteed me disks of three or four feet, and since that time they have offered to undertake a five-foot disk.

One obstinate fact, and one I could get no solution of, was the failure of the four-foot French speculum. But in spite of this failure, a 37-inch glass disk was decided on, to be of about $4\frac{1}{2}$ inches thickness.

There was the question, too, of silvering. Mirrors are usually silvered by being suspended face downwards; but I was quite sure that it could be done face upwards and without even taking it from its cell, and that I could thus avoid a great difficulty with respect to silvering.

I had long been in the habit of silvering mirrors face upwards when testing them during the figuring, as it saved both time and trouble, and I consequently devised a plan for silvering the large mirror which unfortunately was the cause of the blowing up of the first disk.

The cell was perforated as if for a Gregorian. I was going to cut a $2\frac{1}{2}$-inch hole through the glass, and make a gutta-percha plug to fit. This hole was to serve to let off the water and solutions when silvering. A band of stiff paper, coated with something not acted on by the chemicals, was to form a band to hold the solutions. I had tried many things; beeswax is good when pure, but is mostly sold adulterated with oil and rosin; solid paraffin was found the best of all.

I made a strong machine suited to my method of working. I had flatted the back of the disk and got the concave ready for fining, and was, at the same time, edging it and cutting the hole through the centre; but when less than $\frac{1}{4}$ inch in depth had been cut, it burst into hundreds of fragments. The breaking, doubtless, was owing to the disturbance of internal tension.

I was not sure whether removing the skin from the edge or disturbing the centre was the cause of the blowing up, and I therefore tested this question on an $18\frac{1}{2}$-inch disk, which, on attempting to cut a hole through it, blew up also. I have since had a large disk *cast* with a centre in it, and it answers perfectly; and had the 37-inch been cast in the same manner, it would have answered the proposed plan for silvering.

The 37-inch was ground and polished with a tool 36 inches in diameter and weighing nearly 3 cwt., and the focus—17 feet $7\frac{1}{2}$ inches—out within an inch or two of what was intended.

Before polishing was commenced, a portion of the workshop was covered with calico to exclude dust, room only being left for working. A wooden tunnel, commencing at the door and extending 40 feet on the ground level, was covered with sailcloth to keep out light and air-currents.

The machine was made so that its revolving table could be turned from the horizontal to the vertical position; the speculum was worked in its cell; the whole weighing nearly 11 cwt.

When testing commenced I sat in this dark tunnel to test at the centre of curvature, and the speculum was focussed by an assistant with screws, so that I received in my eye the image of a very minute pinhole in a lamp screen close by my head.

During the polishing and figuring I carefully studied the behaviour of the disk—for flexure—for distortion of figure by contraction and expansion during changes of temperature—and I found the disk as perfect as a 6-inch one, and without a single infirmity. I soon found it would admit of a perfect and permanent figure, and it gave in the early stage a perfect and symetrical image of an artificial star, which was magnified many hundred times by an eye-piece. These results were gratifying and satisfactory; they at once removed all doubts of ultimate success.

The work of correcting was tedious and trying, especially in the latter stages, when for every few minutes' polishing the whole preparations for testing had to be repeated, and the settling of the mass into its normal state had to be patiently waited for, and often days passed before further advance could be made.

When the figure had advanced as far as necessary for testing at the centre of curvature, the wooden structure was thrown open at the outer end for the purpose of testing on a distant terrestrial object.

The test objects for daylight purposes consisted of a hole punched through a sheet of metal, with a reflector so placed at the back as to reflect the light of the clouds through, so that it appeared as a bright white spot: a spot of whiting and printed paper was used for definition. When the Sun shone I obtained its image by means of a prism with one of its surfaces polished to a small spherical curve. This was made use of because I could not use a bulb, having to look in the direction of the Sun, and not with the Sun behind me.

For testing at night a lamp covered with a metal screen with a hole through it was used. For every advance towards the correct figure it was first tested at the centre of curvature, and then on the distant object. In the latter case it was, of course, used as a telescope proper, by placing a plane at its working focus for parallel rays.

The plan of figuring was that of local figuring and correcting. The polish and surface was obtained with the large and heavy polisher, and corrected with a number of polishers of various sizes and forms to suit every stage of the progress and the tem-

perature of the air &c. If an error of irregularity of figure set in, it was polished out with the large polisher.

The machine I employed was on a principle which was a modification of that used by the Earl of Rosse. I have made five different machines, one of which was on the principle used by Mr. Lassell—an excellent principle; but I have long since come to the conclusion, that no machine can do the final work like the trained hand, and I was gratified to learn, when in conversation with Professor Draper, that his experience agreed precisely with my own on this point.

The fact that, when polishing specula, we use a tool that is somewhat elastic (for there is nothing equal to pitch as a material for the polisher) shows that it is the *form of the polisher* we want to aim at, and the curve of the glass will follow. Therefore the machine, its rates of revolution and strokes, the size and weight of the polishers, their consistency (which depends on the temperature in which they are worked, and the friction) should all be arranged so as to give to the polisher the figure or curve we desire to give the glass surface.

I may state that the 37-inch disk, as an experimental one, answered all expectations and fulfilled all necessary conditions; and I may also state, as the result of my experience, that I see no obstacle to the construction of glass mirrors of very large sizes.

The 37-inch was silvered by Martin's process, and in this way:—

The surface was washed with some of the potash solution, it was then rinsed and sponged with a handful of pure cotton wool, and finally rinsed.

A band of stout brown paper was ready; that part of it to go next the edge for about two turns was painted with hot melted paraffin. This paper band was wound round, and tightly bound round with strong cord, leaving a rim standing up about two inches above the surface to hold the solutions. Water was then poured on to cover and keep the surface till the bath was ready.

The solutions were filtered through cotton wool. I make the solutions very strong; that is, I dissolve the chemicals in $\frac{1}{4}$ or $\frac{1}{8}$ the quantity of water usually given. Thus the filtering is quickly done and the bulk is small. The quantity of water required to make the bath of sufficient depth can easily be added. The bath was about one inch deep at the edge.

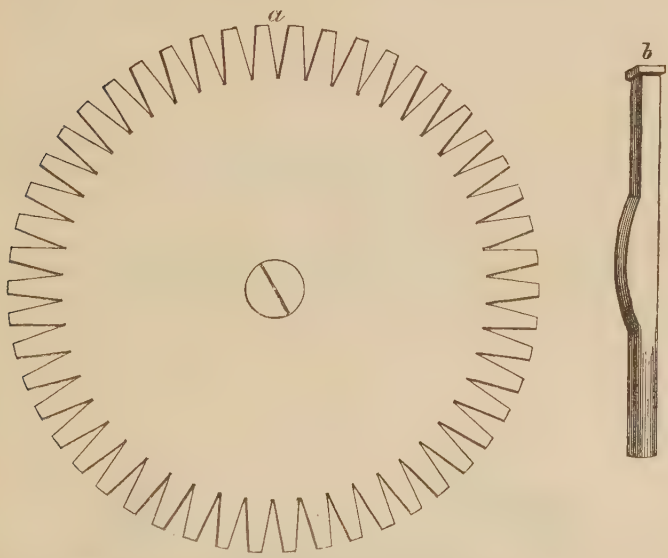
When all was ready, the speculum was tilted and the water shot off; quickly brought to the level again, and the solutions poured on. The sinking particles are thus subtracted by filtration, and the light particles will float harmlessly, and nearly all the silver goes to the glass surface. It was well silvered in twenty minutes, then well washed and sponged over with a handful of cotton wool, finally rinsed, left to dry and polish. It was conveniently silvered with the help of one assistant.

Description of a Self-Registering Micrometer. By Mr. A. Bowden.

The general shape of this instrument is that of an ordinary micrometer, with the following modifications.

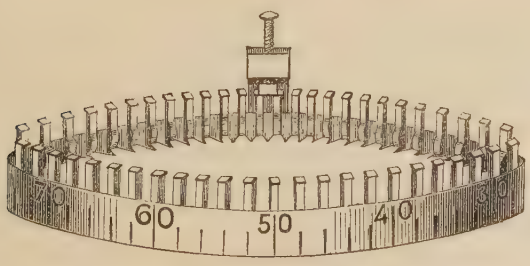
The graduated band has a plate within its circumference at both upper and lower edges; through which are drilled holes immediately within the band and behind each alternate division, so that in a centesimally divided micrometer there are fifty pairs of holes. Through each of these pairs runs a pin (called an *indicator*) shaped as in Fig. 3, *b*, with the points towards the milled

Fig. 3.



head of the micrometer, and the heads standing up on the same side as, and just behind, the index. Through the arm which carries the index, and just behind it, is drilled a hole, through

Fig. 2.



which passes a pin (called the *motor*), having a flat round head

with a square base, and which is kept up, so that the base shall just clear the indicator heads, by a small spiral spring.

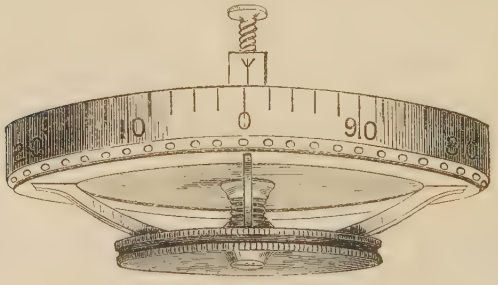
Fig. 2 represents the micrometer head (with the top plate off) ready for use.

When a bisection has been made, the observer presses gently on the head of the motor, thereby driving down the indicator below it, and making its point, which was previously flush with the outer surface of the lower plate, protrude below the band, where it remains. On releasing the motor it springs up, and a fresh bisection may be made. When a sufficient number of bisections have been made, the observer notes the integral number of revolutions in the usual way, and the fractional parts, to hundredths, are taken down from the divisions behind which the indicators protrude.

The relative sizes of the base of the motor and the heads of the indicators are such that, if the micrometer is set so that the index points to any reading not more than half a division on either side of the even division behind which the indicator stands, one indicator only will be driven down; but if the reading is beyond this on either side, two will be projected, and the division between these two will be recorded.

To restore the indicators to their normal positions, a flat ring is carried on the shaft of the micrometer, between the band and the milled head (Fig. 1). This is held down towards the milled

Fig. 1.



head by a spiral spring, and, when pressed up, pushes the indicators back into position. To keep them in position, whether up or down, a thin steel plate, shaped as in Fig. 3, *a*, is fixed between the two plates of the micrometer head, so that an edge of a tooth comes behind each indicator. When the indicator is pressed down, the small protuberance behind it bears on the tooth, which yields and allows it to pass, but immediately straightens and prevents its return till the pressure is given at the other end by the ring described above, when the same action occurs in the opposite direction.

In a micrometer head having a diameter of two inches, the distance between the centres of the indicators would be nearly

$0^{in}\cdot1$. If it is possible to make their distances as little as $0^{in}\cdot05$, so as to have an indicator for each division, readings would be recorded to within $0^{r}\cdot0025$; but even supposing it can only be read to $0^{r}\cdot005$, which, if 1^{r} = say $20''$, would give $0''\cdot1$, this, though not quite so exact as reading from the vernier, practically as accurately records the position of the star in the field, and possesses the advantage of allowing the bisection to be repeated 3, 5, or 7 times.

Radcliffe Observatory, Oxford.

Observations of the Spectrum of Comet 1879 *d* (*Palisa*). By Lord Lindsay.

The spectrum of this comet was observed at Dun Echt on two nights by Ralph Copeland and J. G. Lohse. It consisted of the usual three bright bands, the middle one being the brightest, while the one of shortest wave-length was by much the faintest. The resulting wave-lengths are:

	Sept. 23. mmm.	No. of Obs.	Oct. 10. mmm.	No. of Obs.
Band 1, centre	552·7	(3)	549·8	(1)
,, 2, towards red edge	513·1	(1)	...	...
,, ,, centre	511·0	(1)	512·4	4)
,, 3 ,,	468·9	(3)	462·1	(1)

Dun Echt, 1879, *Nov.* 11.

Observations of Comet 1879 *d* (*Palisa*), *made at Dun Echt Observatory with the Filar Micrometer of the* 15-*inch Refractor.*

By Lord Lindsay.

Date. 1879.	Dun Echt Mean Time. h m s	$\Delta\alpha$ ($\delta - *$) m s	$\Delta\delta$ ($\delta - *$) ′ ″	$\alpha\delta$ h m s	$\delta\delta$ ° ′ ″	Observer.
Aug. 26	11 2 47·1	+3 47·98	−1 54·5	10 35 22·22	+48 19 31·8	J. G. L.
„ 27	12 14 46·3	−5 3·25	−2 55·7	10 42 24·31	48 5 2·5	R. C.
„ 28	15 15 35·4	− 1·84	−2 48·8	10 50 0·55	47 47 46·4	J. G. L.
Sept. 3	14 40 58·2	−1 39·18	−6 45·8	11 30 46·95	45 39 40·6	R. C.
„ 4	13 48 14·8	− 35·68	+3 42·8	11 37 20·26	45 13 12·1	J. G. L.
„ 13	10 59 11·7	−3 10·02	−4 50·9	12 36 7·39	39 51 10·3	R. C.
„ 23	7 53 47·3	− 34·01	−7 25·3	13 34 13·24	31 29 40·7	„
„ 25	8 27 41·5	+ 28·53	−7 19·0	13 44 55·30	29 33 30·6	J. G. L.
Oct. 2	8 45 24·0	− 59·71	+1 32·0	14 18 52·07	22 32 15·1	R. C.
„ 3	7 41 3·8	+ 34·91	+5 51·9	14 23 7·22	21 33 46·9	J. G. L.
„ 6	7 38 35·5	− 24·25	+5 42·1	14 35 57·24	18 30 6·9	„
„ 19	6 54 3·3	...	+ 52·1	...	5 48 11·7	R. C.
„ „	7 5 40·0	+2 1·60	...	15 23 42·26	...	„
„ 20	7 2 8·5	+ 8·69	−3 28·6	15 26 56·66	+ 4 52 59·1	J. G. L.

Adopted Mean Places of Comparison Stars, 1879·0.

	α h	α m	α s	Reduction. s	δ °	δ ′	δ ″	Reduction. ″	
Aug. 26	10	31	32·28	+1·96	+48	21	36·6	−10·3	Arg.-Oeltz. 11006.
„ 27	10	47	25·66	1·90	48	8	8·6	10·4	„ 11221.
„ 28	10	50	0·50	1·89	47	50	45·7	10·5	$\frac{\text{Arg.-Oeltz. 11258} + * \text{ No. 99 } \textit{Ast. Nach.} \text{ No. 538}}{2}$.
Sept. 3	11	32	24·37	1·76	45	46	37·4	10·9	$\frac{\text{Arg.-Oeltz. 11940} + * \text{ No. 128 } \textit{Ast. Nach.} \text{ No. 538}}{2}$.
„ 4	11	37	54 18	1·75	45	9	40·3	11·0	$\frac{\text{Arg.-Oeltz. 12025} + * \textit{ Ast. Nach.} \text{ No. 995}}{2}$.
„ 13	12	39	15·80	1·61	39	56	11·8	10·6	Greenwich Nine-Year 1175.
„ 23	13	34	45·58	1·66	31	37	16·0	10·0	$\frac{\text{Lalande 25259} + \text{B. W. xiii. 685} + \text{B. W. xiii. 686}}{3}$.
„ 25	13	44	25·08	1·68	29	40	59·5	9·9	$\frac{\text{B. W. xiii. 921} + \text{B. W. xiii. 922}}{2}$.
Oct. 2	14	19	50·00	1·78	22	30	52·3	9·2	B. W. xiv. 405.
„ 3	14	22	30·51	1·80	21	28	4·4	9·3	B. W. xiv. 457.
„ 6	14	36	19·64	1·85	18	24	33·9	9·1	B. W. xiv. 746.
„ 19	15	21	38·57	2·09	5	47	28·4	8·8	Lamont $\frac{1730+1731}{2}$.
„ 20	15	26	45·86	2·12	4	56	36·3	8·6	Micrometrically measured from B. W. xv. 501.
(„ 20)	15	28	31·62	+2·12	+5	8	15·6	−8·5	B. W. xv. 501; not used as comparison star.

Note.—Rounding off the above quantities has in some cases given rise to discordances of one unit in the last figure.

Remarks
- Sept. 23. Comet about 3′ diameter, *vgmb*M; but no nucleus with power 307.
- Oct. 19. Comet bright, round, gradually much brighter in the middle; no nucleus.
- Oct. 20. Comet very low; images woolly and unsteady.

On the Evidence of a past Connection between four widely separated Southern Stars. By E. J. Stone, M.A., F.R.S.

I have found it desirable, in the formation of my Cape Catalogue, to examine the cases in which large proper motions have been assigned to Southern Stars in the British Association Catalogue. In the greater number of cases these large proper motions have not been confirmed; but in the following group of stars the proper motions are large and fairly well determined. These proper motions appear to me otherwise interesting, and to indicate the existence, in the past, of some close connection between stars which are now widely separated.

The following data are extracted from the Cape Catalogue now passing through the press:—

Name.	Mag.	R.A. 1880. h m s	Precess. s	P.M. s	N.P.D. 1880. ° ′ ″	Precess. ″	P.M. ″
ζ Toucani	4·	0 13 48·60	+2·8953	+0·280	155 34 49·03	−20·018	−1·13
e Eridani	4·5	3 15 8·16	+2·1169	+0·266	133 31 46·81	−13·214	−0·75
ζ^1 Reticuli	5·6	3 15 9·95	+1·0954	+0·194	153 2 5·58	−13·212	−0·65
ζ^2 Reticuli	5·6	3 15 36·39	+1·0980	+0·190	152 57 53·21	−13·184	−0·65

The angular proper motions of these stars are 2″·13, 2″·99, 1″·47, and 1″·45 respectively. It will be remarked that the proper motions are exceptionally large in both elements, and that they have the same signs for all the stars. In my opinion these facts are not sufficient to prove that the stars have belonged to a stellar system, but they undoubtedly raise a strong presumption that such may have been the case.

We have observations of these stars extending over a period of nearly 130 years. To avoid the complications introduced by changes in the axes of reference, I shall refer the motion of each star to the other stars, and discuss the changes of angular distance.

1st.—For the distances of the close pair of stars ζ^1 and ζ^2 *Reticuli* we have

Epoch.	Distance. ° ′ ″	
1750	0 5 5	Lacaille.
1825	0 5 8·08	Brisbane.
1840	0 5 9·27	Maclear.
1880	0 5 8·40	Stone.

From which we deduce for the epoch $1880+t$

° ′ ″ ″

Distance = 0 5 9·39 + 0·03 × t, when Lacaille's observations are used.

Distance = 0 5 8·79 + 0·0059 × t, when Lacaille's observations are excluded.

Whilst the proper motions of these stars, therefore, have carried each of them through an angular space of 189″, the angular distance of the stars has changed but very little indeed. The slight change which has taken place shows that the angular separation of these stars was formerly less than at the present time. There are only one or two stellar parallaxes at present known as large as 0″·5; but with such a parallax as this these stars must, in the last 130 years, have been carried progressively through at least 34,700,000,000 miles without any corresponding separation of their centres.

2nd.—For the distances between the star *e Eridani* and the mean position of the two stars ζ^1, ζ^2 *Reticuli*, we have

Epoch.	Distance. ° ′ ″
1750	19 28 2·55
1825	19 28 8·00
1840	19 28 8·70
1880	19 28 12·65

From these we deduce for the epoch $1880+t$

	°	′	″	″
Distance =	19	28	12·29	$+0\cdot0762\times t$, including Lacaille.
Distance =	19	28	12·57	$+0\cdot0877\times t$, excluding Lacaille.

The rate of separation between the star *e Eridani* and the centre of gravity of ζ^1 and ζ^2 *Reticuli* is very small compared with the proper motions of the stars themselves, but the distance is increasing.

The star *e Eridani* therefore participates in the large proper motion of ζ^1 and ζ^2 *Reticuli*, but is very slowly separating from that system.

3rd. For the distance between ζ *Toucani* and ζ^1 and ζ^2 *Reticuli* we have

Epoch.	Distance. ° ′ ″
1750	19 25· 16
1825	(19 25 6)
1840	19 25 15
1880	19 25 18

The distance deduced from Brisbane's observations is discordant. The right ascensions of Brisbane's Catalogue are exceedingly rough, and the distance is here greatly affected by errors in right ascension. The general result, however, is the same whether Brisbane's observations be included or excluded. Rejecting Brisbane's distance, we have for the epoch $1880+t$

	°	′	″	″
Distance =	19	25	17	$+0\cdot006\times t$, including Lacaille's observations.
Distance =	19	25	18	$+0\cdot075\times t$, from the Cape observations alone.

The distance is increasing, but very slowly. The distance is nearly the same as in case (2) and the velocity of separation is the same if determined from the Cape observations which are alone corrected for proper motions to the epochs.

In this case also we have the stars participating in a large common proper motion, but slowly separating from each other.

4. The following are the distances between the stars ζ *Toucani* and *e Eridani*:—

Epoch.	Distance.	Obs.—Comp.
	° ′ ″	″
1750	33 4 31	+0·9
1825	33 4 56·5	−0·6
1840	33 5 2·0	−0·5
1880	33 5 16·9	0·0

From which we obtain for the epoch $1880+t$

$$\text{Distance} = 33^\circ\ 5'\ 16''{\cdot}9 + 0''{\cdot}36 \times t.$$

The distance is increasing as in the other cases considered. The rate of change is larger than in the other cases, but it is still much smaller than the common proper motion of the group, and the distance itself is much greater than in the cases (1), (2), and (3.)

It appears therefore—

α. The four stars of the group under consideration have proper motions much larger than the average proper motions of stars.

β. That the stars have a common proper motion of more than a second of arc.

γ. That each star of the group is moving away from every other star of the group by quantities which are small compared with the common proper motion of the group.

δ. That, roughly speaking, the velocities of separation are larger, the larger the present angular separation of the stars.

The two stars ζ^1 and ζ^2 *Reticuli* have been shown to be separating very slowly indeed, and it has then been shown that the triangle formed by joining ζ *Toucani*, *e Eridani*, and the mean position of ζ^1 and ζ^2 *Reticuli* is slowly expanding. It is clear, therefore, that in the remote past the stars of the group must have been much closer together than at present. By a stellar system I mean a group of stars in which the several stars are bound together by much closer gravitational bonds than those which connect the members of one system with the members of another system. Have the stars in the group under consideration ever formed such a stellar system? It appears to me at least exceedingly probable that such must have been the case; but I fear the solution of the problem cannot be carried much further than I have already done.

If we suppose two stars to pass through a point and to

separate by moving uniformly along two straight lines, then the distance d can be expressed as follows:—

$$\tan d = \frac{at + bt^2}{1 + \alpha t + \beta t^2}.$$

We have no knowledge of the time t at which the angular separation of stars is supposed to be small, and the formula therefore contains five unknown quantities. But our observations, which only extend over 130 years, are fairly represented by a formula only containing tha first power of t. We are therefore only really given the distance d and its rate of change for the present epoch. If the stars have ever been sufficiently close to form a stellar system, our accurate astronomy is too much a thing of yesterday to furnish data for the determination of the time at which this must have taken place. The points to which I have called attention appear, however, to point clearly in the direction indicated; but, with respect to time, all we can safely say is that the stars cannot have been sufficiently near to sensibly affect each other's motions within the last 300,000 years.

5. I shall next give the distances, for the epoch $1880 + t$, between the star β *Hydri* and the group of stars considered above.

The following data are extracted from my Cape Catalogue:—

1880.

Name.	Mag.	R.A.	Precess.	P.M.	N.P.D.	Precess.	P.M.
		h m s		s	° ′ ″		″
β Hydri	3	0 19 25·32		+0·720	167 55 48·68		−0·32

The distances between β *Hydri* and

	°	′	″	″
ζ Reticuli =	20	1	12	$-0·33 \times t$;
ζ Toucani =	12	21	24·63	$+0·892 \times t$;
e Eridani =	38	29	33	$-0·44 \times t$.

The star β *Hydri* has therefore a large velocity of separation from ζ *Toucani*, and is moving towards ζ *Reticuli* and e *Eridani*. β *Hydri* cannot therefore have been a member of the same stellar system as ζ *Reticuli*, ζ *Toucani*, and e *Eridani*.

It will be remarked, however, that β *Hydri* has large proper motions in right ascension and north polar distance of the same signs as those of the stars contained in the group under consideration, and that these conditions are therefore not alone sufficient to prove that stars whose proper motions satisfy them belong to a system.

I have examined several other cases, but I have not met with any other stars belonging to the same stellar system.

If we assume the masses of these fourth and fifth magnitude stars to be equal to that of our Sun, and the annual parallaxes $= 0''·5$, the equation of *vis viva* will show that no changes of

proper motion affecting the tenths will have taken place since the angular separation of the stars amounted to a couple of minutes. It appears to me that such a system as that under consideration in the present paper may have originated from a star, or a system of two stars like α^1 and α^2 *Centauri*, with a large proper motion being brought, by reason of that large proper motion, sufficiently near another system to disturb the relative motions of each, and that both systems have thus been changed from close into open orbits. I consider that we now see each star moving with the common motion of the centre of gravity combined with the velocity due to its motion about the centre of gravity of the whole system, which latter velocity has for a long period past been sensibly an uniform velocity in a straight line. It would be interesting to examine the annual parallaxes of these stars.

1879, *October* 25.

Conjunction of Mars and Saturn, June 30, 1879, *observed at the Observatory, Melbourne.* By R. L. J. Ellery.

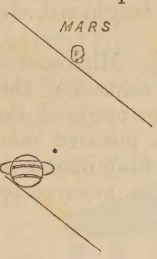
Observations with South Equatoreal, 8″ Aperture, and Parallel Wire Micrometer. Power 250.

Melbourne Mean Time.				Distance by Micrometer.	Distance, corrected, of Semi-diam.	Position of *Mars*.	
	h	m	s	″	″	°	′
June 30	16	25	33	113·19	110·50	294	37
		48	3	99·85	97·25	312	24
		58	19	94·99	92·39	322	24
	17	2	22	93·18	90·58	327	42
		6	37	91·28	88·68	331	12
		12	32	91·66	89·06	336	4
		14	12	90·52	87·92	338	7
		16	20	90·13	87·53	...	
		19	40	90·33	87·73	...	
		22	29	90·71	88·11	347	52
		24	41	91·47	88·87	...	
		27	0	91·47	88·87	...	
		28	25	92·23	87·63	354	2
		32	19	94·14	91·54	...	
		33	38	94·71	92·11	357	42
		35	35	95·85	93·25	359	22
		38	42	97·38	94·78	...	
		43	11	102·52	99·92	4	22

Remarks.—The measures having been made from south limb of *Mars* to south limb of *Saturn*, and the first column of distances corrected for wire zero only, a correction of −2″·60 for diff. of semi-diameter is applied, which gives the distances in second column. No correction for parallax or refraction has been applied.

Observations by Mr. Ellery with the 8-inch Refractor.

Measures with Parallel-wire Micrometer, power 250.
Colour Comparison with negative eye-piece, 220.



Saturn.—*Green yellow* or *green cream colour*, sombre and dull in tone. *Divisions in ring* and *dark band* on body of planet well marked. Four satellites visible during first measures; but only three, as shown (two of which assumed to be *Titan* and *Rhea*), during the latter part of the observation.

Mars.—Very red-yellow, especially at terminator near North Pole. Greenish dark spot about centre of planet. *South Polar Ice Cap* like a bead of bright silver apparently projecting beyond the limb.

Colours of planets contrast strongly in the field of the telescope. *Saturn's* colour quite remarkable.

Measures were made from south (bright) limb of *Mars* to south limb of *Saturn*, and diameter of *Saturn's* ball measured (mean of 5 measures) 16″·1.

The sky was perfectly clear throughout the observations. Atmosphere quiet and steady; images sharp, well defined, and undisturbed.

Observations by Mr. Turner with the Great Telescope.

Saturn is of a pale greenish yellow tint.

Mars is a bright orange-red. Gibbous in shape, the bright limb of *Mars* being nearest to *Saturn*, thus:

The illuminated limb of *Mars* is of a bright reddish yellow—the more central and far-off portions of a bright yellow without red. The North Pole of *Mars* is of a *bright* reddish yellow—the South Pole of a *very pale yellow*, or whitish yellow tone.

The colours of *Mars* are altogether *much more brilliant* than that of *Saturn*. *Saturn* has a dull sickly appearance, but *Mars* is bright and brilliant.

In the Finder the contrast of colour is, if anything, more striking than in the Great Telescope.

One dark band is seen on *Saturn*, below the ring, and three satellites, as shown above.

Sky beautifully clear, atmosphere steady, and altogether a splendid morning for such an observation.

Powers used 235 and 255.

Observations by Mr. White with the North Equatoreal of $4\frac{1}{2}$ inches aperture and 60 inches focal length.

At $16^h\ 55^m$ examined the planets in a dark field, with negative eye-piece magnifying 140 times. *Mars* looked bright red, slightly tinged with orange. *Saturn* pale buff colour.

At $17^h\ 0^m$ put in Simms's Position Wire Micrometer and measured the apparent distances between the centres of the illuminated disks of the planets and the position angle of the line joining them. The eye-piece used was a positive one and magnified 82 times; the field was brightly illuminated during these measures. The following are the results, uncorrected for parallax or refraction:—

Melbourne Mean Time. h m s	Angle of Position. ° ′	Distance. ″	Melbourne Mean Time. h m s	Angle of Position. ° ′	Distance. ″
17 3 25	327 5	...	17 23 1	347 55	88·2
4 28	329 53	89·9	25 28	349 51	88·8
7 3	...	90 2	27 56	353 0	89·9
8 59	333 9	90 1	30 13	354 52	89·9
11 3	337 39	87·1	32 41	357 37	91·6
12 55	338 26	86·4	35 29	0 3	94·5
15 33	340 15	88 0	39 54	2 51	95·9
17 45	343 9	87·7	17 43 52	6 31	99·5
20 44	345 39	88·7	18 6 38	21 22	121·9

The mean height of the barometer during the observations was $30^{in}\cdot259$, attached thermometer $48°\cdot5$ Fahr. and temperature of the air $35°\cdot0$ Fahr.

The Astronomer Royal presented to the Society a series of 28 Photographs of Drawings of *Mars*, made at the Royal Observatory, Greenwich, during the Opposition of 1877.